Personal Memo from
JOSHUA LEDERBERG

Seymour Chen

DEC 29 1986

PSB draft... Stanley etc

Dear Seymour -

I have no comments now, except thanks; if more come to mind I'll write again.

* In my limited encounters with Stanley I never thought he had much of an institution for biological or genetic. Perhaps that's why he was undaunted by the idea of "crystallizing life."

In like vein, as he later admitted, he was very critical of Avery and doubtless played a critical role in Avery not being recognized by the Nobel Prize committee.

(*nor CA Knight!)

P.S. I just went to my bookshelf to look up WMS's Nobel lecture in David Baltimore's compendium on the Prizes in Mol. Biol. Of course it's not included.

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Seymour Chen re W Stanley.

these are now known to be derived from longer rods broken in the preparation of the sample for microscopy. How homogeneous was a population of virus particles? Lauffer, analyzing the spreading of a sedimenting boundary of tomato busy stunt virus, concluded that the diameters of the particles could deviate from the mean by no more than 1%. In an exuberant moment, Lauffer referred to "living molecules."

Nevertheless, it is of considerable interest that neither group tested the infectivity of viral RNA before 1956. Despite the availability of appropriate viral RNA after 1936 and inactivating crystalline ribonuclease in 1940, despite the demonstration of DNA as Pneumococcal transforming agent in 1944 and the apparent infectivity of phage DNA, accepted by the community of phage workers in 1952 after Hershey and Chase {what did the model have to do with it} ~~in 1953 following the discovery of the Watson-Crick model,~~ the thought that the viral RNA might be the genetic element of this virus was not tested before 1956.

In 1940 E. Pfankuch et al. had studied X-ray induced mutations of the virus and had attributed differences in the phosphorus contents of the parent and mutant strains to irradiation-induced alterations in the nucleic acid part of the virus. These data were not considered convincing in 1941 by C.A. Knight and Stanley who had found differences in the amino acid compositions of various strains. They had concluded that "the chemical differences between strains probably lies not in the nucleic acid but rather in the protein part of the virus molecule." They apparently did not consider the possibility that the nucleic acid might determine the composition of the protein. Following this line of thought, Miller and Stanley* modified amino acid residues with a variety of reagents but found that, although many groups could be modified without loss of biological activity, the virus propagated was normal virus. At this point in the work, early in the entrance of the United States into World War II, the

* see comm note.